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perhaps not solely) to a stretching of the muscle preliminary to its contraction. He prefaces the statement of his own work by a brief historical summary.

Ueber den Muskelsinn. GOLDSCHIEDER. Verhandl. d. physiol. Gesells. Berlin. Sitzung am 17 Mai, 1889.

The hypothesis of a sensation of motion, distinct from that of the positions beginning and ending the movement, is supported by the following considerations. 1. The sensation of motion becomes clearer as the rapidity of the movement increases, and attends movements of too short duration for the complicated processes of a judgment from the positions. 2. The just observable sensation of motion accompanies movements so very small that their limiting positions are probably indistinguishable. 3. Sensations of motion are clearly perceived before the direction of movement is clear. 4. The sensation of position can be temporarily removed by faradizing without destroying the sensation of motion.

In experiments on the lifting of weights it is well to use only single segments of the limb. The physiological conditions of the experiment are thus greatly simplified, with the result that in lifting the weight by a thread nothing of the sensation of encountering at some moment the resistance of an exterior heavy object is felt (a prominent sensation in lifting with more than one segment,) but only the more subjective sensation of heaviness (*Schwere-Empfindung*), of greater difficulty in executing the previously easy movement. This sensation of heaviness has its seat in the tendons; that of resistance, like that of motion, in the joints, and is called forth by the pressure of the joint surfaces upon one another. It suffers if there is motion in the joint at the same time. In lifting weights in the ordinary way both sensations are aroused.

As against the participation of an innervation sense in these judgments the following facts are adduced. 1. The sensation of weight is felt when the contraction of the muscle is produced by electrical stimulation or reflexly like the knee-jerk. 2. The limb may seem perfectly relaxed when it is still partly supported by muscular tension. 3. Movements may be made actively as well as passively which are too small to be perceived by the subject, and the limit of perceptible movements is raised by faradization in one case as in the other. 4. Innervation sensations do not mediate the sense of position, for that is almost entirely destroyed by faradization. 5. Certain illusions exist which should not be possible with an innervation sense. The consciousness of voluntary movement comes from the immediate succession of peripheral sensations of motion upon the genesis of the corresponding motor image. (The weight of evidence at present is very strongly against the existence of innervation sensations, and those whose theory of space perception involves them will have to bethink themselves of reconstruction. REV.)

Zur Frage der psychophysischen Messungen bei Geisteskranken. M. K. WALITZKAJA. Archiv f. klin. u. gerichtl. Psychiatrie v. Merschejewski. I, 17. Rev. by Kraepelin in the Allg. Zeitsch. f. Psychiatrie. Bd. XLVI, H. 2-3, S. 245*.

These experiments were made on 7 insane and, for comparison, on 5 sane subjects. In 4 of the 7 cases the diagnosis was general paralysis, in 2 progressive paralysis, in 1 paranoia. Simple reaction-

times, choice-times, association-times, etc., and the times for adding digits were measured to a total number of 18,000. A table of the mean times required for these processes is given, also a table in which the association-times are further classified. But the very grave defects of method, (those which Kraepelin enumerates touch the control of the apparatus, the making of the protocol and the presentation of the results) rob the work of scientific value. The conclusions at which the author arrives are, in general, that in the beginning of paralysis there is an increase of automatic mental activity (including association) together with weakened volition; at a later stage the automatic activity also fails by degrees and simple perception is difficult. In nearly complete remission the automatic activities return fully, the voluntary and intellectual only partially; when the disease becomes acute the automatic processes again become prominent, the volitional difficult.

These results are not so very different from those found in the same field by Madam M. K. Walicka.

On some Facts of Binocular Vision. J. VENN. *Mind*. April, 1889.
 "On some Facts of Binocular Vision." J. H. HYSLOP. *Mind*. July, 1889.

Prof. Venn finds himself unable to get some of the experimental results brought forward by Dr. Hyslop in a previous paper, (reviewed, *AMER. JOUR. PSY.* II, 159.) In his reply the latter shows these differences to be more apparent than real and by no means important to the central idea of his paper, which was a criticism of Wundt's innervation theory. The discussion demonstrates as Dr. Hyslop notices, the great complexity of such experiments and the large part which personal variations in skill may have in the results.

L'agrandissement des astres à l'horizon. G. LECHALAS. *Revue Philosophique*, Juillet, 1888.

Une association inséparable: l'agrandissement des astres à l'horizon. M. BLONDEL. *Ibid.*, Nov., 1888.

Continuation of the discussion, *Ibid.*, Déc., 1888 and Fév., 1889.

Why does the moon look large at the horizon? The current explanation is that it then seems large because it seems far away, and seems far away because many things intervene. The object of Lechalas's paper is to review this theory in the light of certain experiments of Stroobant's (*Bul. de l'Acad. de Belg.* VIII (1884), 719; X (1885), 315). The most important of these were three. (1) He projected an after-image (of the sun in this case) upon a wall at such a distance that the after-image seemed of the same size as the sun, and found that distance to be always about 48 m. Plateau, who seems to have originated the experiment, found 51 m. with the moon. This shows, Stroobant contends, that these bodies always appear at the same apparent distance, and is therefore a definite disproof of the distance theory. Lechalas objects that the moon really seems much further than 50 m., and Blondel shows in his paper that while the experiment shows the constancy of the retinal image, it does not show the constancy of the apparent distance, appearances being for the mind and not the eye. (2) The second experiment, the force of which Lechalas admits without abatement and to which he assigns a physiological explanation, was as follows: The experimenter produced near the ceiling of a perfectly dark room two